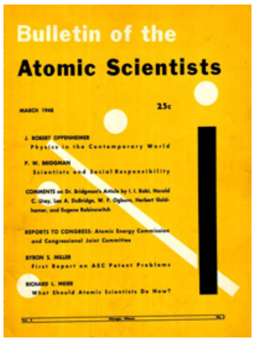

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PHYSICS IN THE CONTEMPORARY WORLD

J. Robert Oppenheimer

Dr. Oppenheimer's authority to speak on the relation of physics to the contemporary scene derives from his years of experience as a teacher of physics, his leadership in the Manhattan Project and his current position as Director of the Institute for Advanced Study at Princeton. The following remarks formed the 1947 Arthur D. Little Memorial Lecture and are reprinted from THE TECHNOLOGY REVIEW, February, 1948, edited at the Massachusetts Institute of Technology.

In the ways of science, its practice, the peculiarities of its discipline and universality, there are patterns which in the past have somewhat altered, and in the future may greatly alter, all that we think about the world and how we manage to live in it. What I shall be able to say of this will not be rich in exhortation; for this is ground that I know how to tread only very slightly.

But that I should be speaking of such general and such difficult questions at all reflects in the first instance a good deal of self-consciousness on the part of physicists. This self-consciousness is in part a result of the highly critical traditions which have grown up in physics in the last half century, where we have been forced to become aware of what it is that we are doing. It reflects also the experiences of this century, which have shown in so poignant a way how much the applications of science determine our welfare and that of our fellows, and which have cast in doubt that traditional optimism, that confidence in progress, which have characterized Western Culture since the Renaissance.

As it did on everything else, the last war had a great and at least a temporarily disastrous effect on the prosecution of pure science. The demands of military technology in this country and in Britain, the equally over-riding demands of the Resistance in much of Europe, distracted the physicists from their normal occupations as they distracted most other men.

We in this country, who take our wars rather spastically, perhaps witnessed a more total cessation of true professional activity in the field of physics, even in its training, than any other people. For in all the doings of war we, as a country, have been a little like the young physicist who went to Washington to work for the NDRC in 1940. There he met his first Civil Service questionnaire, and came to the questions on drinking—never, occasionally, habitually, to excess. He checked both "occasionally" and "to excess." So, in the past we have taken war.

All over the world, whether because of the closing of universities, or the distractions of scientists called in one way or another to serve their countries, or because of devastation and terror and attrition, there was a great gap in physical science. It has been an exciting and an inspiring sight to watch the recovery: a recovery testifying to extraordinary vitality and vigor in this human activity. Today, barely two years after the end of hostilities, physics is booming.

One may have gained the impression that this boom derives primarily from the application of the new techniques developed during the war, such as the atomic reactor and micro-wave equipment; one may have gained the impression that in large part the flourishing of physics lies in exploitation of the eagerness of governments to promote it. These are indeed important factors. But they are only a small part of the story. Without in any way deprecating the great value of wartime technology, one nevertheless sees how much of what is today new knowledge can trace its origin directly, by an orderly yet imaginative extension, to the kind of things that physicists were doing in their laboratories and with their pencils almost a decade ago.

Let me try to give a little more substance to the physics that is booming. We are continuing the attempt to discover, to identify and characterize, and surely ultimately to order, our knowledge of what the elementary particles of physics really are. I need hardly say that in the course of this we are learning again how far our notion of *elementarity*, of what makes a particle elementary, is from the early atomic ideas of the Hindu and Greek atomists, or even from the chemical atomists of a century ago. We are finding out that what we are forced to call elementary particles retain neither permanence nor identity, and they are elementary only in the sense that their properties cannot be understood by breaking them down into sub-components. Almost every month has surprises for us in the findings about these particles. We are meeting new ones for which we are not prepared. We are learning how poorly we had identified the properties even of our old friends among them. We are seeing what a challenging job the ordering of this experience is likely to be, and what a strange world we must enter to find that order.

Tools for Understanding Elementary Particles

In penetrating into this world perhaps our sharpest tool in the past has been the observation of the phenomena of the cosmic rays in interaction with matter. But the next years will see an important methodological improvement, when the great program of ultra high energy accelerators begins to get under way. This program is itself one of the expensive parts of physics. It has been greatly subsidized by the Government, primarily through the Atomic Energy Commission and the Office of Naval Research. It is a superlative example, of which one could find so many, of the repayment that technology makes to basic science, in providing means whereby our physical experience can be extended and enriched.

Another progress is the refinement of our knowledge of the behavior of electrons within atomic systems, a refinement which on the one hand is based on the micro-wave techniques, to the developments of which the Radiation Laboratory of the Massachusetts Institute of Technology made unique contributions, and which on the other hand has provided a newly vigorous criterion for the adequacy of our knowledge of the interactions of radiation and matter. Thus we are beginning to see in this field at least a partial resolution, and I am myself inclined to think rather more than that, of the paradoxes that have plagued the professional physical theorists for two decades.

A third advance in atomic physics is in the increasing understanding of those forces which give to atomic nuclei their great stability, and to their transmutations their great violence. It is the prevailing view that a true understanding of these forces may well not be separable from the ordering of our experience with regard to elementary particles, and that it may also turn on an extension to new fields of recent advances in electro-dynamics.

However this may be, all of us who are physicists by profession know that we are embarked on another great adventure of exploration and understanding, and count ourselves happy for that.

The International Picture

In how far is this an account of physics in the United States only? In how far does it apply to other parts of the world, more seriously ravaged and more deeply disturbed by the last war? That question may have a somewhat complex answer, to the varied elements of which one may pay respectful attention.

In much of Europe and in Japan, that part of physics which does not rest on the availability of elaborate and radical new equipment is enjoying a recovery comparable to our own. The traditional close associations of workers in various countries makes it just as difficult now to disentangle the contributions by nationality as it was in the past. But there can be little doubt that it is very much harder for a physicist in France, for instance, or the Low Countries, and very much more nearly impossible for him in Japan, to build a giant accelerator, than for the workers in this country.

Yet in those areas of the world where science has not merely been disturbed or arrested by war and by terror, but where terror and its official philosophy have, in a deep sense, corrupted its very foundations, even the traditional fraternity of scientists has not proved adequate protection against decay. It may not be clear to us in what way and to what extent the spirit of scientific inquiry may come to apply to matters not yet, and perhaps never to be, part of the domain of science; but that it does apply there is one very brutal indication. Tyranny, when it gets to be absolute, or when it tends so to become, finds it impossible to continue to live with science.

Even in the good ways of contemporary physics, we are reluctantly made aware of our dependence on things which lie outside our science. The experience of the war, for those who were called upon to serve the survival of their civilization through the Resistance, and for those who contributed more remotely, if far more decisively, by the development of new instruments and weapons of war, has left us with a legacy of concern. In these troubled times it is not likely that we shall be free of it altogether. Nor perhaps is it right that we should be.

Nowhere is this troubled sense of responsibility more acute, and surely nowhere has it been more prolix, than among those who participated in the development of atomic energy for military purposes. I should think that most historians would agree that other technical developments, notably radar, played a more decisive part in determining the outcome of this last war. But I doubt whether that participation would, of itself, have created the deep trouble and moral concern which so many of us who were physicists have felt, have voiced, and have tried to get over feeling. It is not hard to understand why this should be so. The physics which played the decisive part in the development of the atomic bomb came straight out of our laboratories and our journals.

Despite the vision and the farseeing wisdom of our wartime heads of state, the physicists felt a peculiarly intimate responsibility for suggesting, for supporting, and in the end, in large measure, for achieving, the realization of atomic weapons. Nor can we forget that these weapons, as they were in fact used, dramatized so mercilessly the inhumanity and evil of modern war. In some sort of crude sense which no vulgarity, no humor, no over-statement can quite extinguish, the physicists have known sin; and this is a knowledge which they cannot lose.

The Interaction of Science and Technology

Probably in giving expression to such feelings of concern most of us have belabored the influence of science on society through the medium of technology. This is natural, since the developments of the war years were almost exclusively technological, and since the participation of academic scientists forced them to be deeply aware of an activity of whose existence they had always known but which had been often remote from them.

When I was a student at Göttingen twenty years ago, there was a story current about the great mathematician Hilbert, who perhaps would have liked, had the world let him, to have thought of his science as something independent of worldly vicissitudes. Hilbert had a colleague, an equally eminent mathematician, Felix Klein, who was certainly aware, if not of the dependence of science generally on society, at least of mathematics on the physical sciences which nourish it and give it application. Klein used to take some of his students to meet once a year with the engineers of the Technical High School in Hanover. One year he was ill, and asked Hilbert to go in his stead, and urged him, in the little talk that he would give, to try to refute the then prevalent notion that there was a basic hostility between science and technology. Hilbert promised to do so; but when the time came a magnificent absentmindedness led him instead to speak his own mind: "One hears a good deal nowadays of the hostility between science and technology. I don't think that is true, gentlemen. I am quite sure that it isn't true, gentlemen. It almost certainly isn't true. It really can't be true. *Sie haben ja gar nichts mit einander zu tun.* They have nothing whatever to do with one another." Today the wars and the troubled times deny us the luxury of such absentmindedness.

The great testimony of history shows how often in fact the development of science has emerged in response to technological, and even economic needs, and how in the economy of social effort, science, even of the most abstract and recondite kind, pays for itself again and again in providing the basis for radically new technological developments. In fact, most people, when they think of science as a good thing, when they think of it as worthy of encouragement, when they are willing to see their governments spend substance upon it, when they greatly do honor to men who in science have attained some eminence, have in mind that the conditions of their life have been altered just by such technology, of which they may be reluctant to be deprived.

The debt of science to technology is just as great. Even the most abstract researches owe their very existence to things that have taken place quite outside of science, and with the primary purpose of altering and improving the conditions of man's life. As long as there is a healthy physics, this mutual fructification will surely continue. Out of its work there will come in the future, as so often in the past, and with an apparently chaotic unpredictability, things which will improve man's health, ease his labor, and divert and edify him. There will come things which, properly handled, will shorten his working day and take away the most burdensome part of his effort, which will enable him to communicate, to travel, and to have a wider choice both in the general question of how he is to spend his life, and in the specific question of how he is to spend an hour of his leisure. There is no need to belabor this point, nor its obverse—that out of science there will come, as there has in this last war, a host of instruments of destruction which will facilitate that labor, even as they have facilitated all others.

The Scientist Is Responsible to Science

But no scientist, no matter how aware he may be of these fruits of his science, cultivates his work, or refrains from it, because of arguments such as these. No scientist can hope to evaluate what his studies, his researches, his experiments, may in the end produce for his fellowmen, except in one respect—if they are sound, they will produce knowledge. And this deep complementarity between what may be conceived to be the social justification of science, and what is for the individual his compelling motive in its pursuit, makes us look for other answers to the question of the relation of science to society.

One of these is that the scientist should assume responsibility for the fruits of his work. I would not argue against this, but it must be clear to all of us how very modest such assumption of responsibility can be, how very ineffective it has been in the past, how necessarily ineffective it will surely be in the future. In fact, it appears little more than an exhortation to the man of learning to be properly uncomfortable; and, in the worst instances, is used as a sort of screen to justify the most casual, unscholarly and, in the last analysis, corrupt intrusion of scientists into other realms of which they have neither experience nor knowledge, nor the patience to obtain it.

The true responsibility of a scientist, as we all know, is to the integrity and vigor of his science. And because most scientists, like all men of learning, tend in part also to be teachers, they have a responsibility for the communication of the truths which they have found. This is at least a collective if not an individual responsibility. That we should see in this any insurance that the fruits of science will be used for man's benefit, or denied to man when they make for his distress or destruction, would be a tragic naiveté.

Can Scientists' Ways Provide a Pattern?

There is another side of the coin. This is the question of whether there are elements in the way of life of the scientist which need not be restricted to the professional, and which have hope in them for bringing dignity and courage and serenity to other men. Science is not all of the life of reason; it is a part of it. As such, what can it mean to man?

Perhaps it would be well to emphasize that I am talking neither of wisdom, nor of an élite of scientists, but precisely of the kind of work and thought, of action and discipline, that makes up the everyday professional life of the scientist. It is not of any general insight into human affairs that I am talking. It is not the kind of thing we recognize in our greatest statesmen, after long service devoted to practical affairs and to the public interest. It is something very much more homely and robust than that. It has in it the kind of beauty that is inseparable from craftsmanship and form, but that has in it also the vigor which we rightly associate with the simple ordered lives of artisans or of farmers, that we rightly associate with lives to which limitations of scope, and traditional ways, have given robustness and structure.

Even less would it be right to interpret the question of what there is in the ways of science which may be of general value to mankind in terms of the creation of an élite. The study of physics, and I think my colleagues in the other sciences will let me speak for them too, does not make philosopher-kings. It has not, until now, made kings. It almost never makes fit philosophers—so rarely that they must be counted as exceptions. If the professional pursuit of science makes good scientists, if it makes men with a certain serenity in their lives, who yield perhaps a little more slowly than others to the natural corruptions of their time, it is doing a great deal, and all that we may rightly ask of it. For if Plato believed that in the study of geometry, a man might prepare himself for wisdom and responsibility in the world of men, it was precisely because he thought so hopefully that the understanding of men could be patterned after the understanding of geometry. If we believe that today, it is in a much more recondite sense, and a much more cautious one.

What then is the point? For one thing it is to describe some of the features of the professional life of the scientist, which make it one of the great phenomena of the contemporary world. Here again, I would like to speak of physics; but I have enough friends in the other sciences to know how close their experience is to ours. And I know too that despite profound differences in method and technique, differences which surely are an appropriate reflection of the difference in the areas of the world under study, what I would say of physics will seem familiar to workers in other disparate fields, such as mathematics, or biology.

The Nature of the Scientific Discipline

What are some of these points? There is, in the first instance, a total lack of authoritarianism, which is hard to comprehend or to admit unless one has lived with it. This is accomplished by one of the most exacting of intellectual disciplines. In physics the worker learns the possibility of error very early. He learns that there are ways to correct his mistakes; he learns the futility of trying to conceal them. For it is not a field in which error awaits death and subsequent generations for verdict—the next issue of the journals will take care of it. The refinement of techniques for the prompt discovery of error serves as well as any other as a hallmark of what we mean by science.

In any case, it is an area of collective effort, in which there is a clear and well defined community whose canons of taste and order simplify the life of the practitioner. It is a field in which the technique of experiment has given an almost perfect harmony to the balance between thought and action. In it we learn so frequently that we could almost become accustomed to it, how vast is the novelty of the world, and how much even the physical world transcends in delicacy and in balance the limits of man's prior imaginings. We learn that views may be useful and inspiriting although they are not complete. We come to have a great caution in all assertions of totality, of finality or absoluteness.

In this field quite ordinary men, using what are in the last analysis only the tools which are generally available in our society, manage to unfold for themselves and all others who wish to learn, the rich story of one aspect of the physical world, and of man's experience. We learn to throw away those instruments of action and those modes of description which are not appropriate to the reality we are trying to discern, and in this most painful discipline, find ourselves modest before the world.

Can This Discipline Be Applied to Other Fields?

The question which is so much in our mind is whether a comparable experience, a comparable discipline, a comparable community of interest, can in any way be available to mankind at large. I suppose that all the professional scientists together number some one one-hundredth of a per cent of the men of the world—even this will define rather generously what we mean by scientists. Scientists as professionals are, I suppose, rather sure to constitute a small part of our people.

Clearly, if we raise at all this question which I have raised, it must be in the hope that there are other areas of human experience that may be discovered or invented or cultivated, and to which the qualities which distinguish scientific life may be congenial and appropriate. It is natural that serious scientists, knowing of their own experience something of the quality of their profession, should just today be concerned about its possible extension. For it is a time when the destruction and the evil of the last quarter century make men everywhere eager to seek all that can contribute to their intellectual life, some of the order and freedom and purpose which we conceive the great days of the past to have had. Of all intellectual activity, science alone has flourished in the last centuries, science alone has turned out to have the kind of universality among men which the times require. I shall be disputed in this; but it is near to truth.

If one looks at past history, one may derive some encouragement for the hope that science, as one of the forms of reason, will nourish all of its forms. One may note how integral the love and cultivation of science was with the whole awakening of the human spirit which characterized the Renaissance. Or one may look at the late Seventeenth and Eighteenth Centuries in France and England, and see what pleasure and what stimulation the men of that time derived from the growth of physics, astronomy and mathematics.

What perhaps characterizes these periods of the past, which we must be careful not to make more heroic because of their remoteness, was that there were many men who were able to combine in their own lives the activities of a scientist with activities of art and learning and politics, and were able to carry over from the one into the others this combination of courage and modesty which is the lesson that science always tries to teach to anyone who practices it.

Can the Spirit of Science Be Taught?

And here we come to a point we touched earlier. It is very different to hear the results of science, as they may be descriptively or even analytically taught in a class or in a book or in the popular talk of the time; it is very different to hear these and to participate even in a modest way in the actual attainment of new knowledge. For it is just characteristic of all work in scientific fields that there is no authority to whom to refer, no one to give canon, no one to blame if the picture does not make sense.

Clearly these circumstances pose a question of great difficulty in the field of education. For if there is any truth in the views that I have outlined, there is all the difference in the world between hearing about science or its results, and sharing in the experience of the scientist himself and of that of the scientific community. We all know that an awareness of this, and an awareness of the value of science as method, rather than science as doctrine, underlies the practices of teaching to scientist and layman alike. For surely the whole notion of incorporating a laboratory in a high school or college is a deference to the belief

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January 29. Working committee heard Mr. Miles observe, on Soviet Point 3, that "the inspectorial apparatus is about all the agency will have." M. de Rose said the agency must have other functions. Detailed discussion was left to later linked points.

Point 4 came up, providing for a special control convention "to be concluded in accordance with the convention on the prohibition of atomic weapons." With it was a British question asking if the Soviet would agree that the outlawry pact "shall only come into force following satisfactory implementation of the second convention." The Soviet had answered that the control convention "must be concluded" after conclusion of the prohibition convention.

M. de Rose observed the 1907 Hague convention had prohibited use of poison gas, but such gas was used in World War I and manufactured between the two world wars by "all powers." He said all points should be dealt with simultaneously by a convention.

Mr. Miles recalled the 1925 Geneva protocol, signed by some thirty countries, prohibited both gas and bacteriological warfare. "Nevertheless, there is not a large state in the world that has not conducted active research into the use of these forms of warfare, and the world well knows that it cannot depend on this convention." Prohibition without prevention is useless, he said.

Dr. John D. Babbitt, of Canada, a National Research Council physicist, felt there was room for clarifying the relation between the two Soviet conventions. He asked for a "yes or no" answer to the British suggestion that the prohibition convention should come into force following implementation of the control pact.

Mr. Gromyko Refuses Overtures

Mr. Gromyko slammed the door: "We consider that the convention on the prohibition of atomic weapons must be not only signed but also put into force before the other convention is concluded." Otherwise, he said, the outlawry pact "would not make sense" and would be "a piece of paper."

Chairman Farris el Khouri, of Syria, asked if this meant destruction of existing bombs before the control pact, too. Mr. Gromyko said it did—within the period specified by the Soviet's June 19, 1946 draft convention proposal, namely, within ninety days after ratification.

Dr. Wei asked if, in case the first convention were carried out and then majority and minority proposals on controls developed: "Will the minority go along with the majority?" He asked if there were any assurance of a second convention. He feared lest the first step become "the last step," and the prohibition pact "meet the same fate—failure—as many previous conventions."

Mr. Gromyko said it had not been the Soviet fault that the commission had not yet found a basis for agreement. The Soviet had pointed out decreases in divergences on "certain minor points," and "then some members of the Atomic Energy Commission did everything possible to widen this divergence of views on certain points, and we could not help coming to the conclusion that probably not all of us are in agreement at the present time at least on carrying out in practice the decision of the United Nations on the establishment of effective international control of atomic energy."

The Soviet delegate added: "Such proposals as were submitted by the United States representative—I have in mind the so-called Baruch plan, which, as far as I know, was not substantially changed later by the United States—are absolutely unrealistic.

"They do not follow from the interests and demands of the establishment of effective international control. They are subordinated to the interests mainly of one country . . . We have to carry out our negotiations on the basis of realistic and practical proposals, and put aside those proposals which prove to be unrealistic and which cannot constitute a basis for agreement."

Chairman Khouri said the commission should not waste further time in "academic discussion," but should prepare both prohibition and control conventions simultaneously, as quickly as possible. "If they were then presented together," he said, "there would, I feel, no longer be any pretext for opposition or contention." He expressed the belief the Soviet would not object to the control convention if it were presented and put into force at the same time as the prohibition convention.

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that not only what the scientist finds but how he finds it is worth learning and teaching and worth living through.

Yet there is something fake about all this. No one who has had to do with elementary instruction can have escaped a sense of artificiality in the way in which students are led, by the calculations of their instructors, to follow paths which will tell them something about the physical world. Precisely that groping for what is the appropriate experiment, what are the appropriate terms in which to view subtle or complex phenomena, which are the substance of scientific effort, almost inevitably are distilled out of it by the natural patterns of pedagogy. The teaching of science to laymen is not wholly a loss; and here perhaps physics is a typically bad example. But surely they are rare men who, entering upon a life in which science plays no direct part, remember from their early courses in physics what science is like or what it is good for. The teaching of science is at its best when it is most like an apprenticeship.

President Conant, in his sensitive and thoughtful book, *On Understanding Science*, has spoken at length of these matters. He is aware of how false it is to separate scientific theory from the groping, fumbling, tentative efforts which lead to it. He is aware that it is science as method and not as doctrine which we should try to teach. His basic suggestion is that we attempt to find, in the history of our sciences, stories which can be recreated in the instruction and experiment of the student, and which thus can enable him to see at first hand how error may give way to less error, confusion to less confusion, and bewilderment to insight.

The problem that President Conant has here presented is indeed a deep one. Yet he would be quite willing, I think, that I express skepticism that one can recreate the experience of science as an artifact. And he would no doubt share my concern that science so taught would be corrupt with antiquarianism. It was not antiquarianism, but a driving curiosity, that inspired in the men of the Renaissance their deep interest in classical culture.

For it is in fact difficult, almost to the point of impossibility, to recreate the climate of opinion in which substantial errors about the physical world, now no longer entertained, were not only held but were held unquestioned as part of the obvious mode of thinking about reality. It is most difficult to do because in all human thought only the tiniest fraction of our experience is in focus, and because to this focus a whole vast unanalyzed account of experience must be brought to bear. Thus I am inclined to think that with exceptions that I hope will be many, but fear will be few, the attempt to give the history of science as a living history will be far more difficult than either to tell of the knowledge that we hold today, or to write externally of that history as it may appear in the learned books. It could easily lead to a sort of exercise of mental inventiveness on the part of teachers and students alike which is the very opposite of the candor, the "no-holds-barred" rules of Professor Bridgman, that characterize scientific understanding at its best.

If I am troubled by President Conant's suggestions, this is not at all because I doubt that the suggestions he makes are desirable. I do have a deep doubt as to the extent to which they may be practical. There is something irreversible about acquiring knowledge; and the simulation of the search for it differs in a most profound way from the reality. In fact, it would seem that only those who had some first-hand experience in the acquisition of new knowledge in some disciplined field would be able truly to appreciate how great the science of the past has been, and would be able to measure those giant accomplishments against their own efforts to penetrate a few millimeters further into the darkness that surrounds them.

Thus it would seem at least doubtful that the spiritual fruits of science could be made generally available, either by the communication of its results, or by the study of its history, or by the necessarily somewhat artificial reenactment of its procedures. Rather it would seem that there are general features of the scientists' work, the direct experience of which in any context could contribute more to this end. All of us, I suppose, would list such features and find it hard to define the words which we found it necessary to use in our lists. But on a few, a common experience may enable us to talk in concert.

In the first instance the work of science is cooperative; a scientist takes his colleagues as judges, competitors and collaborators. The work of science is disciplined, in that its essential inventiveness is most of all dedicated to means for promptly revealing error. One may think of the rigors of mathematics, and the virtuosity of physical experiment as two examples. Science is disciplined in its rejection of questions that cannot be answered, and in its grinding pursuit of methods for answering all that can. Science is always limited, and is in a profound sense unmetaphysical, in that it necessarily bases itself upon the broad ground of common human experience, tries to refine it within narrow areas where progress seems possible and exploration fruitful. Science is novelty, and change. When it closes it dies . . . These qualities constitute a way of life which of course does not make wise men from foolish, or good men from wicked, but which has its beauty and which seems singularly suited to man's estate on earth.

If there is to be any advocacy at all in this talk it would be this—that we be very sensitive to all new possibilities of extending the techniques and the patterns of science into other areas of human experience.

The Scientific Approach and Social Problems

We become fully aware of the need for caution if we look for a moment at what are called the social problems of the day, and try to think what one could mean by approaching them in the scientific spirit, of trying to give substance, for example, to the feeling that a society that could develop atomic energy could also develop the means of controlling it. Surely the establishment of a secure peace is very much in all our minds. It is right that we try to bring reason to bear on an understanding of this problem; but for that there are available to us no equivalents of the experimental techniques of science. Errors of conception can remain undetected and even undefined. No means of appropriately narrowing the focus of thinking is known to us. Nor have we found good avenues for extending or deepening our experience that bears upon this problem. In short, almost all the preconditions of scientific activity are missing, and in this case, at least, one may have a melancholy certainty that man's inventiveness will not rapidly provide them. All that we have from science in facing such great questions is a memory of our professional life, which makes us somewhat sceptical of other people's assertions, somewhat critical of enthusiasms so difficult to define and to control.

Yet the past century has seen many valid and inspiring examples for the extension of science to new domains. One feature which I cannot fail to regard as sound—particularly in the fields of biology and psychology—is that they provide an appropriate means of correlating understanding and action, and involve new experimental procedures in terms of which a new conceptual apparatus can be defined; above all, they give us means of detecting error. In fact, one of the features which must arouse our suspicion of the dogmas some of Freud's followers have built up on the initial brilliant works of Freud, is the tendency towards a self-sealing system, a system, that is, which has a way of almost automatically discounting evidence which might bear adversely on the doctrine. The whole point of science is to do just the opposite: to invite the detection of error and to welcome it. Some of you may think that in another field a comparable system has been developed by the recent followers of Marx.

Thus we may hope for an ever widening and more diverse field of application of science. But we must be aware how slowly these things develop, and how little their development is responsive to even the most desperate of man's needs. For me it is an open question, and yet not a trivial one, whether in a time necessarily limited by the threats of war and of chaos, these expanding areas in which the scientific spirit can flourish may yet contribute in a decisive way to man's rational life.

I have had to leave this essential question unanswered: I am not at all proud of that. In lieu of apology perhaps I may tell a story of another lecturer, speaking at Harvard two decades ago. Bertrand Russell had given a talk on the then new quantum mechanics, of whose wonders he was most appreciative. He spoke hard and earnestly. And when he was done, Professor Whitehead, who presided, thanked him for his efforts, and not least for "leaving the vast darkness of the subject unobscured."